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Use of Modern Medical Care for Pregnancy and Childbirth Care:

Does Female Schooling Matter?

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Abstract

Controversy exists over whether the estimated effects of schooling on health care use reflect the influence of unobserved factors. Existing estimates may overstate the schooling effect because of the failure to control for unobserved variables or may be downwardly biased due to measurement error. This paper contributes to the resolution of this debate by adopting an instrumental variable approach to estimate the impact of female schooling on maternal health care use. A school construction program in Indonesia in the 1970s is used to construct an instrumental variable for education. The choice between use and non-use of maternal health services is estimated as a function of schooling and other variables. Data from the Indonesia Family Life Survey are used for this paper.

Standard regression models estimated in the paper indicate that each additional year of schooling does indeed have a significant, positive effect on maternal health care use. Instrumental variable estimates of the schooling effect are larger. The results suggest that schooling has a positive impact on maternal health care use even after eliminating the effect of unobserved variables and measurement error.

This paper moves beyond previous work on the impact of education on health care use by adopting an IV approach to address the problem of endogeneity and measurement error. IV methods have been used widely in the labour economics literature to examine the impact of schooling on wages and other labour market outcomes but rarely to estimate the effect of schooling on health outcomes.

This paper—a product of the Health, Nutrition and Population Unit, Human Development Sector Department of East Asia and Pacific Region—is part of a larger effort in the department to disseminate working papers by staff members on issues related to human development. This paper was written as part of the author's doctoral thesis submitted to Harvard University in June 2006. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at asomanathan@worldbank.org.

USE OF MODERN MEDICAL CARE FOR PREGNANCY AND CHILDBIRTH CARE: DOES FEMALE SCHOOLING MATTER?

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1. Introduction

The impact of schooling on health care use has been examined in a wide range of settings and found to be important. Controversy exists, however, over whether, and to what extent, the estimated returns to schooling reflect the influence of unobserved preferences, knowledge, and motivation of individuals. To the extent that schooling outcomes reflect such attributes, existing estimates may overstate the schooling effect because of the failure to control for background variables. An alternative argument is that estimates of the schooling effect may be downwardly biased due to measurement error in the schooling variable. This paper contributes to the resolution of this debate by adopting an instrumental variable (IV) approach to estimate the impact of female schooling on maternal health care use in Indonesia during the 1990s. A large-scale school construction program that took place in Indonesia in the 1970s, the Sekolah Dasar INPRES program (SDIP) is used to construct an instrumental variable for education. Standard regression estimates presented here indicate that each additional year of schooling does indeed have a significant, positive effect on the timing of ante-natal care visits, use of hospital care and skilled assistance at delivery. IV estimates of the schooling effect are larger. The results suggest that schooling has a positive impact on maternal health care use even after eliminating the effect of unobserved variables and measurement error. The analysis also highlights several other correlates of poor maternal health use such as low levels of access to services and household socioeconomic status, which could potentially offset the impact of schooling.

This paper moves beyond previous work on the impact of education on health care use by adopting an IV approach to address the problem of endogeneity and measurement error. Doing so enables it to produce unbiased estimates of the magnitude of the impact of education on health care use. IV methods have been used widely in the labor economics literature to examine the impact of schooling on wages and other labour market outcomes (Angrist and Krueger 2000), but have rarely been used to estimate the effect of schooling on health outcomes. Along with Breierova and Duflo (2004), this study represents one of the first attempts to use an IV method to estimate the effect of schooling on health care utilization and outcomes.

A second contribution of this paper is to provide a more accurate assessment of the impact of education on health care use. Public policy has generally emphasised the role of female schooling as a primary driver of preference change. Empirical research has shown that education is major determinant but has been ambivalent about the impact of its effect and the extent to which it may be overestimated. Failure to recognise the limits of education as a tool for changing preferences, with respect to health care use, may result in public policy based on unrealistic expectations from mothers, slow rates of improvements in maternal and child health and, consequently, an increased burden on women. This paper does not seek to identify the effects of other factors driving changes in women’s preferences for health care use. It does, however, provide an assessment of the size of the schooling effect relative to other key determinants of health care use such as household socioeconomic status and access to health services.

I begin with an overview of the complex relationship between education and health care use, explanations as to why the relationship is endogenous and a review of the empirical evidence on this subject. Section 3 contains a description of the school construction program, which forms the basis of the instrument used in this paper. Research hypotheses and contributions of the proposed research are presented in Section 4. Section 5 describes the data and Section 6 outlines the analytical framework, including the proposed identification strategy. Section 7 presents the empirical analysis and results. Section 8 reviews the findings in light of existing evidence on the determinants of maternal health care use and evaluates the instrumental variable approach used here.

2. Education and maternal health care

Pathways of influence and potential endogeneity

The health transitions literature has devoted considerable attention to conceptualizing the pathways through which education affects health care use, beginning with Caldwell and others in 1970s (Caldwell 1979; Cleland and van Ginneken 1988; Caldwell 1990). In the economics literature, the human capital framework which forms the basis of much of the economic analysis of education and health care use (Becker 1965; Grossman 1972; Grossman 2000) identifies pathways that are largely similar to those of Caldwell et al.

Education primarily affects care seeking behavior by altering women's predisposing characteristics in a number of ways. Firstly, education imparts a greater responsiveness to new ideas and services, more social confidence in handling outside officials including health professionals, greater decision-making power within the household and an enhanced ability and willingness to travel outside the home community in search of services, all of which give rise to a greater propensity to seek care outside the home when sick. By making individuals less fatalistic about diseases and death and more knowledgeable about illness causation and the effectiveness of modern treatment, education also changes norms and beliefs. Secondly, education changes individuals' propensity to perceive illness, leading them seek care in a more timely manner for their family members and themselves. More educated individuals are likely to extract a higher quality of care from the services they receive and adhere to advice with greater persistence, thus improving the efficiency with which the available health inputs and technologies are used (Michael 1973; Rosenzweig and Schultz 1985). Changes to the predisposing characteristics affect individual and household preferences for health care independently of prices, income and information (Caldwell 1979).

A second channel through which the education effect operates is through its impact on the enabling resources available to individuals and households. Women's schooling increases the economic resources available to the family through assortive mating with wealthier men, through increased earnings associated with market efficiency gains (Schultz 1984; Ware 1984) and through an increase in full incomes due to non-market efficiency gains (Michael 1973). In addition, being employed enhances women's control over household economic resources, enabling them to allocate a greater share of household resources towards their own health care and nutrition.

The impact of education on the use of pregnancy and childbirth care services is endogenously determined because education is highly correlated with numerous other determinants of care-seeking behavior. For instance, education is correlated with the individual's family background, her parents' and siblings' own education, employment status and area of residence which, in turn, affect her preferences with regard to health care use. In addition, it is correlated with the level of access an individual had while growing up to schools, hospitals and other modern institutions. Education is also associated with the degree of gender stratification in the individual's household and community. In common with all of these correlates are factors which engender in women a greater propensity to break away from traditional ideas, absorb and act upon new ideas, such as the use of modern medical services for pregnancy and childbirth care. These preferences and capabilities are rarely observed and difficult to explicitly include in health care use models. Endogeneity arises because the education variable reflects the effects of these unobserved correlates, as well as the direct effect of schooling itself.

There is a well-established empirical literature on the impact of education on health and the demand for health inputs. Most studies focusing on the impact of education on health itself have found that higher levels of education, particularly maternal education, are associated with improved child health, measured in terms of child mortality and nutrition (Caldwell 1979; Wolfe and Behrman 1982; Wolfe and Behrman 1984; Hossain 1989; Barrera 1990; Bicego and Boerma 1993; Benefo and Shultz 1996; Lavy, Strauss et al. 1996). The proximate determinants of health through which education influences health outcomes have also been empirically examined. Studies have assessed the impact of education on food nutrient intake and sanitary practices (Cebu Study Team 1991); use and timing of prenatal health services and delivery assistance (Behrman and Wolfe 1987; Wong, Popkin et al. 1987; Schwartz, Akin et al. 1988; Elo 1992; Gertler, Rahman et al. 1993; Panis and Lillard 1994; Sandiford, Cassel et al. 1995; Pebley, Goldman et al. 1996; Guilkey and Riphahn 1998); and child immunizations (Steele, Diamond et al. 1996; Gage, Sommerfelt et al. 1997; Guilkey and Riphahn 1998)). Most studies find that maternal education has a positive impact on health care use and that paternal education is less important.

For the most part, existing work on the impact of schooling on health care use has not adequately controlled for unobserved preferences and capabilities that are correlated with schooling. A few studies have attempted to control for omitted variable bias by using data on the mother's family to control for family fixed effects (Wolfe and Behrman 1987; Strauss 1990). Wolfe and Behrman's study in Nicaragua found that once the mother's fixed effect is removed using data on her siblings, the association between a mother's schooling and child health disappears. In a separate paper, Berhman and Wolfe (1987) examine the impact of schooling on women's and children's nutrition, health care use and outcomes after controlling for unobserved common childhood family background characteristics. They find that female schooling has a positive impact on women's nutrient and health intakes, even after controlling for unobservables. Using data on extended families living together, Strauss finds that the correlation is reduced once household fixed effects are controlled for.

Other studies have attempted to correct for the endogeneity of schooling by including a range of individual and community level variables in the model as controls and by using fixed effects specifications. Rosenzweig and Schultz (1982) view female schooling and health care as partial substitutes for information about diseases, treatment of illness and child-care practices. Using data from Colombia, Rosenzweig and Schultz control for access to services to show that in areas where services are readily accessible, they are used by both educated and uneducated women; the advantage conferred by schooling on health outcomes is diminished as a result. Desai and Alva (1998) examine the impact of education on child health outcomes and Elo (1992) looks at the effect on use of maternal health care services. They control for urban area of residence, access to water and sanitation services, and include community fixed effects in their models. Both methods reduce the effects of years of schooling. Kravdal (2003) incorporates variables to control for average education and relative autonomy of women at the community level. His main hypothesis is that everyone, including those with relatively low levels of education, can take advantage of the generally high level of education in a community. In general, controlling for confounders produces smaller estimates of the effects of individual schooling.

To-date, only one study has adopted an instrumental variable approach to examining the impact of schooling on health. Breierova and Duflo (2004) take advantage of a school construction program in Indonesia in the 1970s to instrument the effect of education on fertility and child mortality and to compare the effect of mother's schooling relative to the father's education. I use the same instrumental variable in this paper to examine the effect of schooling on maternal health care use.

To summarize, there is a general consensus in the empirical literature that education is an important determinant of the demand for health inputs, including the utilization of health services. It is also accepted that schooling is endogenous and has not been adequately controlled for in the majority of studies (Strauss and Thomas 1995; Lindelow 2004).

3. School construction program in Indonesia

A major school construction program, Sekolah Dasar Inpres Program (SDIP) which was undertaken by the Government of Indonesia (GOI) from 1973/74 onwards forms the basis for the instrumental variable used to measure the effect of education in this study. A brief description of the program follows. The identification strategy based on Duflo (2001) is described in a later section.

GOI undertook a series of large-scale social sector development programs in the 1970s, which were aimed at improving equity across provinces. Oil price increases in the 1970s meant that Indonesia, a major oil exporter in world markets could mobilize oil revenues to finance numerous centrally-administered development programs termed "presidential instructions" (INPRES). SDIP was one of the first INPRES programs and, by far, the largest at the time it was launched in 1973/74 (Duflo 2001). As oil prices rose and real expenditures on regional development doubled between 1973 and 1980, SDIP gained in importance. Between 1973-74 and 1978-79 more than 61,000 primary school buildings –

an average of two schools per 1000 children – were built. The total cost was equivalent to 1.5% of Indonesia's GDP in 1973. SDIP has been reported to be one of the fastest primary school construction programs ever undertaken in the world (World Bank 1990).

Once a school was built under the program, the government recruited teachers and paid their salaries. Each school was designed for three teachers and 120 students. The quality of teaching at the new schools did not worsen as efforts to train new teachers took place alongside the establishment of new schools. The stock of schools multiplied by two over the period, and the stock of teachers grew by 43% (Duflo 2001). Prior to 1973, there was a freeze on capital expenditures and teacher recruitment; enrollment rates appeared to be on the decline in the early 1970s (Daroesman 1971; Heneveld 1978). SDIP thus represented a drastic change in Indonesian policy in the education sector.

GOI's goal was to increase enrollment rates among children aged 7 to 12 from 69% in 1973 to 85% by 1978. SDIP was thus designed explicitly to target children who had not previously been enrolled in school. The general allocation rule was that the number of schools to be constructed in each district was proportional to the number of children of primary school age *not enrolled in school* in 1972. From 1975-1976, the allocation rule was altered slightly but implied the same: the number of schools to be constructed was proportional to the number of new students to be accommodated into schools between 1972 and 1978 in order to satisfy the target enrollment rate of 85% in the region by 1972 (Duflo 2000; Duflo 2001). The final allocation of schools to each district was decided by planners in the Ministry of Education and Culture, with the approval of the Department of Finance and Bappenas, the administration responsible for the final implementation of the program. By 1978, the enrollment rate had reached 84% among males and 82% among females (Duflo 2000).

4. Hypotheses

The main objective of this analysis is to measure the impact of schooling on use of formal health services for pregnancy and childbirth care. I test the hypothesis that schooling has a positive impact on the probability of use of three types of maternal care services: at least one ante-natal care visit during the first trimester of pregnancy, skilled assistance at delivery and delivery in a hospital.

This analysis is concerned with the impact of education on a cohort of women aged 1-24 years in 1974, the year SDIP began. However, the analysis of maternal care utilization can only be conducted for a sub-sample of these women who gave birth relatively later as data are not available for earlier births. To the extent that the group of women who began childbearing relatively later in life are also more educated and more exposed to other drivers of modernization and change, the maternal care analysis of the sub-sample may overestimate the real impact of education. To gain an understanding of the degree to which schooling influenced women's fertility, I also analyze the impact of schooling on the timing of fertility choices made by women. I test the hypothesis that schooling leads to an increase in the age at which women start childbearing and has a negative impact on the probability of having children before 21 years of age.

5. Data

This paper focuses on the cohort of women born between 1950 and 1973 and their use of health services for pregnancy and childbirth. Data used for the analysis are from the Indonesia Family Life Survey (IFLS), an ongoing longitudinal survey of individuals, households and communities. Three waves of IFLS have already been conducted in 1993, 1997 and 2000. A total of 10,435 households were surveyed in the most recent wave. The instrument used in all three waves followed a similar structure and repeated the same questions allowing comparison across the waves. Using information collected in the three waves, I have constructed a pooled dataset of all women born between 1953 and 1974 who were included in the IFLS survey. For each woman, the first pregnancy that was reported in the IFLS survey was selected for analysis so that the dataset consists of only one pregnancy for each woman. Women, therefore, form the unit of analysis for this study.

Data used for the analysis are drawn primarily from the Ever-Married Woman Information (EMWI) book of IFLS, which collected retrospective life histories on marriage, children ever born, pregnancy outcomes and contraceptive use from all ever-married women aged 15 to 49 years. The childbirth section of this book collected data on pregnancy-related care, such as type of provider sought for delivery and ante-natal care and, frequency and timing of ante-natal care visits for all pregnancies that took place during the five year period preceding each survey round. As the first round of IFLS was conducted in 1993, detailed health care use data are available for all births that took place from 1988 onwards. As its name implies, the EWMI book collected pregnancy data only for ever-married women in the sample. Any pregnancies to never married women in the cohort would therefore not be observed in IFLS. In the Indonesian context, this is not a major source of bias as only a very small proportion of births occur outside of wedlock.

In each wave of IFLS, respondents provided detailed information on a wide range of demographic, social and economic topics. Information about household level expenditures, assets and income generation activities was collected through interviews with the head of household or his/her spouse. In addition, interviews were conducted with each individual aged 15 and older to collect data on educational, marital, work and migration histories, health and health care use. A proxy book was used for collecting more limited information about individuals who could not be interviewed in person. Data on years of schooling, as well as information on household consumption, employment and marital status were extracted from the different modules of the IFLS survey for the sample of women selected for the analysis. In a separate section of the questionnaire, each household was asked about the distance and travel time to the nearest health center, midwife and hospital. This information was used to construct health care access variables used in the analysis.

Pooling data on women and pregnancies from what is essentially a panel dataset was complicated by inconsistencies in reporting information about pregnancies across different waves. Standard IFLS practice is to collect full retrospective information on pregnancies for all new respondents and to update them on subsequent rounds. Although this rule was generally well-adhered to, in some instances information on the same pregnancies was collected in two or more waves; for some pregnancies, the order of the pregnancy and its outcome were coded differently across waves. Duplication of pregnancies and inconsistencies in coding meant it was necessary to check all pregnancies reported in the pooled dataset individually before dropping duplicate observations and correcting birth order and outcomes data.

Pooling observations made it possible to check the consistency of schooling data reported by panel individuals across the three waves. Years of schooling reported in IFLS were generally consistent between one wave and the next. Where there were inconsistencies, detailed education modules from each wave were compared in order to identify the correct level of schooling for each individual. Information on the highest level of schooling achieved was collected in the household roster, as well as the education module in each wave. This provided an additional level of verification for schooling data.

Summary statistics are provided in Table 1. On average, women had 6.2 years of education, which is slightly above primary school level. At least one ante-natal care visit occurred during the first trimester for just over 70% of all observed pregnancies; 17% of all pregnancies resulted in hospital deliveries. Nearly half of all observed deliveries were assisted by a doctor, nurse of skilled midwife at home or in hospital; the remainder were assisted by traditional birth attendants or family members, generally at home.

Table 1: Summary statistics

	Mean	Standard deviation
Fertility and maternal health care choices		
Share of women who received ante-natal care in the first trimester	0.7067	0.4553
Share of women who had skilled assistance at delivery	0.4856	0.4999
Share of women who delivered in a hospital	0.1731	0.3783
Share of women who had children by age 21	0.4455	0.4971
Average age at first birth	21.03	4.58
Characteristics of the women		
Average years of schooling	6.1922	4.0650
Share of women in each age group in 1974		
1-6 yrs	0.2889	0.4533
7-12 yrs	0.2845	0.4512
13-18 yrs	0.2369	0.4252
>18 yrs	0.1897	0.3921
Share of women in each region of birth		
Sumatera	0.1940	0.3954
Java	0.5516	0.4974
Jakarta	0.0589	0.2354
Bali	0.0463	0.2101
Nusa	0.0594	0.2364
Kalimantan	0.0383	0.1919
Sulawesi	0.0516	0.2211
Share of women from urban households	0.4946	0.5000
Average annual household consumption per capita (Rp.'000,000)	61.49	76.74
Average time to nearest health centre or midwife from home (hrs)	0.3096	0.4023
Age at first marriage	19.06	4.96
Pregnancy-specific characteristics[(a)]		
Average age at childbirth	26.21	5.95
Share of pregnancies at each level of parity		
Reference group: first child	0.3680	0.4823
Low parity: 2nd or 3rd child	0.3522	0.4777
Medium parity: 4th or 5th child	0.1849	0.3883
High parity: 6th child or higher	0.0178	0.2931
N (full sample)	5356	
N (sample with maternal care use data)	3648	

Notes:
(a) These refer to the index pregnancy chosen for the analysis for each woman in the sample

Data on the school construction program in each individual's district of birth were needed to construct the instrumental variable used in this analysis. Data on the *actual* number of schools built in each district between 1973/74 are not readily available from GOI or other sources. The Ministry of Education and Culture does, however, have data on the *planned* allocation of schools to each district for each year between 1973/74 and 1978/79, which are used for this analysis[1]. IFLS migration modules contain information on each individual's district of birth, which I have used to link each individual observation with corresponding SDIP data for her district of birth.

Two data issues related to district of birth data are worth mentioning. The IFLS migration module is only available from the first and third waves of the survey, as the module from the second (1997) wave has not been made publicly available yet. This was not a serious constraint as the majority of women in the sample were observed in at least one of the other two waves for which migration modules are available. However, 300 women who were only observed in the 1997 wave could not be matched with corresponding SDIP data as their district of birth was not known. A second issue is that it is common in Indonesia for district boundaries to be re-drawn and new districts created within districts from year to year, a process which accelerated around 2000, when all government administration in Indonesia was fully decentralized. IFLS migration modules verified changes in district names with respondents and recorded both current and former names for each individual's district of birth. To ensure that birth districts reported in IFLS were

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[1] I am grateful to Esther Duflo for making this dataset available to me.

correctly matched with districts in the SDIP dataset, I compared district names reported in the 1993 and 2000 waves, examined sub-district names and where necessary, referred to detailed district level maps.

District level descriptive statistics of the school construction program are provided in Table 2. IFLS data were representative of only 222 of the 293 districts that existed in Indonesia when the school construction program was launched[2]. Comparison of summary statistics in Panel A (full sample of districts) and Panel B (IFLS sample of districts), indicates that school construction program intensity was slightly lower on average in the IFLS sample of districts than in the country as a whole.

[2] Duflo (2000, 2001) and Beirerova and Duflo (2004) used all 293 districts to construct the instrument as they used Census data for their analysis.

Table 2: School construction programme summary statistics

	Panel A: All districts[a]		**Panel B: IFLS districts[b]**	
	Mean	**Standard deviation**	**Mean**	**Standard deviation**
Total number of districts in sample	293		222	
INPRES schools constructed (1973/74 - 1978/79)	222	173	252	182
INPRES schools constructed per 1000 children (1973/74 - 1978/79)	2.3387	1.2621	2.2105	1.0706
Fraction of population that were children in 1971	0.2868	0.0772	0.2714	0.0176
Enrolment rate in the population in 1971	0.1777	0.0969	0.1689	0.0606
Allocation of water and sanitation programmes in 1973/74	0.7064	0.5680	0.5525	0.2959

Notes:
(a) All districts for which school construction data were available
(b) Districts with school construction data represented in IFLS survey (districts of birth)
Sources:
Data on school construction programme collected by Esther Duflo from INPRES instructions, Census (1971) and Ministry of Education and Culture.

6. Analytical framework

Economic demand model

Empirical analysis of health care choices is generally based on the human capital and household production literature. Grossman (1972a; 1972b) presented a model in which health affects the total time that can be spent productively, instead of having a direct impact on market or non-market productivity. Health, rather than health care, enters the utility function. Health care is included in the production function, along with time and other intermediate inputs including education. The demand for medical care represents the rational response to a health shock, which leads individuals to shift resources away from consumption towards medical care and other health improving inputs (Lindelow 2003). In developing country settings, empirical estimation of the demand for medical care has relied on a simpler, static version of the Grossman framework (Gertler, Locay et al. 1987; Gertler and van der Gaag 1990), which is also adopted here.

Following Lindelow (2003), I adopt a demand framework in which the choice between use and non-use of maternal health services is derived from a simple, random utility model. The respective utility of receiving health services for pregnancy and childbirth (*s*) and not receiving services (*ns*) are represented as,

$$U^s = U(h_s, x_s, \varepsilon_s; \varphi_s) \quad (1)$$

$$U^{ns} = U(h_{ns}, x_{ns}, \varepsilon_{ns}; \varphi_{ns}) \quad (2)$$

where,

h is health status,

x is non-health consumption,

ε is a random error term and

φ is a parameter vector.

Health status is represented as a health production function for each state: use and non-use.

$$h_s = h(edu, \mathbf{z}; \mathbf{b}_s) \quad (3)$$

$$h_{ns} = h(edu, \mathbf{z}; \mathbf{b}_{ns}) \quad (4)$$

where,

edu represents the level of schooling completed by the individual and $\mathbf{z}$, a vector of other individual, household and community attributes.

Choice of maternal health care is represented by the following indicator function:

$$S = 1(U_s > U_{ns}) \tag{5}$$

A woman chooses a particular maternal care alternative if she derives greater utility from it than from not doing so. The trade-off between health and non-health consumption is critical to this model and arises as long as $x_s < x_{ns}$ and $h_s > h_{ns}$.

In line with earlier work on health care demand in developing countries (Akin, Griffin et al. 1986; Mwabu 1986; Schwartz, Akin et al. 1988) and Lindelow (2003) the empirical specification in this paper is based on a linear utility and health production function such that,

$$U^s = \varphi_{s1} h_s + \varphi_{s2} x_s + \varepsilon_s \tag{6}$$

$$U^{ns} = \varphi_{ns1} h_{ns} + \varphi_{ns2} x_{ns} + \varepsilon_{ns} \tag{7}$$

where,

$$h_s = \beta_s^1 edu + \mathbf{b}_s^z{}' \mathbf{z} \tag{8}$$

$$h_{ns} = \beta_{ns}^1 edu + \mathbf{b}_{ns}^z{}' \mathbf{z} \tag{9}$$

Non-health consumption, x is a function of exogenous income, y and travel time, t and is defined as follows.

$$x_s = \gamma_{s1} y - \gamma_{s2} t \tag{10}$$

$$x_{ns} = \gamma_{ns1} y - \gamma_{ns2} t \tag{11}$$

The indirect utilization function is written as follows after appropriate re-parametrization using linear functions of *h* and *x*,

$$V^s = V[\mathbf{a}_s{}'\mathbf{w} + \varepsilon_s] \tag{12}$$

$$V^{ns} = V[\mathbf{a}_{ns}{}'\mathbf{w} + \varepsilon_{ns}] \tag{13}$$

where

$$\mathbf{w} = \begin{bmatrix} \mathbf{z} \\ edu \\ y \\ t \end{bmatrix} \tag{14}$$

The probability of choosing a type of pregnancy or childbirth care is,

$$\Pr[S=1 \mid \mathbf{w}] = \Pr[V_s > V_{ns}] = \Pr[(\mathbf{a}_s - \mathbf{a}_{ns})\mathbf{w} > \varepsilon_{ns} - \varepsilon_s] = \Pr[\mathbf{a}'\mathbf{w} > \varepsilon]$$

with $\mathbf{a} = \mathbf{a}_s - \mathbf{a}_{ns}$ and $\varepsilon = \varepsilon_{ns} - \varepsilon_s$

Under the assumption that $\varepsilon \sim N(0,1)$,

$$\Pr[S=1 \mid \mathbf{w}] = \Pr[\mathbf{a}'\mathbf{w} > \varepsilon] = \Pr[\mathbf{a}'\mathbf{w} < \varepsilon] = \Phi(\mathbf{a}'\mathbf{w}) \tag{15}$$

where Φ is the standard normal distribution. The probability of using maternal health care can, therefore, be estimated using a probit specification.

The dependent variable is a dichotomous variable for whether a woman sought ante-natal care during the first trimester of her pregnancy, whether she delivered at a hospital instead of any other type of institution or home and whether delivery was assisted by a trained health care professional instead of a traditional birth attendant or family member. A separate model is estimated for each of the three dichotomous outcome variables. The main explanatory variables in the model are education (*edu*), income (*y*) and travel time (*t*). Education enters the model as the number of years of schooling completed by each individual. Household consumption per capita is included as a proxy for income because of measurement error in the income variable. For each individual, *t* is measured as travel time in hours to the nearest health center or midwife.

Four variables are included pertaining to the pregnancy that was chosen for the analysis for each woman. They are birth order or parity, mother's age at the time of the pregnancy, whether the previous pregnancy for the same women resulted in a live birth, and the year in which the pregnancy took place. All variables, except mother's age at pregnancy, are dummy variables. The first two are included because very young or very old women, primagravidae and women having high parity pregnancies are more likely to access formal health care because of the risks involved. Birth order of the index pregnancy also captures family size effects associated with health care use, such as the inconvenience of seeking health care services outside home when the mother has concurrent child care responsibilities (Institute of Medicine 1988). The outcome of the previous pregnancy is also likely to determine health care choice. On the one hand, a previous live birth may be indicative of women who are more likely to see care during pregnancy. On the other hand, a previous miscarriage or stillbirth may lead women to seek more care for the subsequent pregnancy. Year of the pregnancy is included to control for time trends in childbirth care services and technology available to women and more general shifts in preferences over time.

In addition, all models control for the woman's cohort of birth. Given increases in the availability of modern health services in the 1980s and 1990s in Indonesia, younger women in the sample are more likely to have had access to modern medicine when they began childbearing. On the one hand, greater accessibility and exposure to modern services could conceivably affect behavior; for instance, older women may be less comfortable with modern medicine and more reluctant to take advantage of available services than younger women. On the other hand, experiences and skills acquired by older women, particularly if they already had several children, would have a positive influence on health care use.

Choice of maternity care is also influenced by a range of unobserved individual preferences and capabilities, which are present in the error term, ε_i. Standard probit analysis assumes that unobserved determinants of maternity care use are not correlated with any of the explanatory variables included in the model in (15) above and are exogenous. With many of the explanatory variables correlated with the unobservables, estimating (15) using a standard probit model would violate the Gauss-Markov assumption that $E(\mu_i \mid x_i) = 0$. Estimated coefficients of the schooling effect would be biased and inconsistent as a result. I use an IV approach to correct for the problem of omitted variable bias in the model. The IV approach also helps correct for potential measurement error in the education variable.

Identification strategy

I construct an instrumental variable for years of schooling, which exploits SDIP, the school construction program carried out during the 1970s, following Duflo (2000; 2001).

An individual's exposure to the school construction program was determined by (i) her age at the time the school program was launched in 1974 and (ii) the number of schools built in her region of birth. Since Indonesian children normally attend primary school between the ages of 7 and 12, all children who were 12 years or older in 1974 (i.e. born in 1962 or earlier) would have already completed primary school by the time the program started and would not have benefited from it. Grade repetition and delayed school entry mean that a few of the children born prior to 1962 could still have been exposed to the program. However, analysis of IFLS data shows that less than 7% of all women born between 1950 and 1962 were still in primary school in 1974. For children born after 1962, exposure to the program is an increasing function of their date of birth. A second dimension of variation in children's exposure to the program is their region of birth. Program intensity in different regions across the country was based on enrollment rates in each region in 1972. Children born in regions where enrollment rates were relatively low in 1971 would have been exposed to a higher level of SDIP intensity than those in regions with higher initial enrollment rates.

Based on these two observations, Duflo (2000; 2001) used the interaction between individuals' age in 1974 and the number of schools built in their region of birth to evaluate the impact of the program. For instance, the difference between education of men who were aged 2 to 6 in 1974 (the exposed cohort) and that of men aged 12 to 17 in 1974 (the unexposed cohort) was 0.47 in regions that got more schools, and 0.36 in regions that got fewer schools. Duflo attributed the difference in differences (0.12) to the program, under the assumption that the increase in years of schooling would not have been systematically different between low and high intensity program regions in the absence of the program itself. She checked this assumption by estimating the same differences in differences for two cohorts who were never exposed to the program and found no effect.

Having controlled for region of birth and cohort of birth fixed effects, interactions between an individual's age in 1974 and SIDP program intensity in the region of birth may be used as instruments to estimate the impact of schooling on use of maternity care services. Using this instrument requires data on two population cohorts: the "control" group of individuals born between 1950 and 1962 and the "exposed" group of individuals born between 1963 and 1974. Women belonging to these two cohorts would have entered their childbearing years during 1988-2000, the period for which IFLS data are available on pregnancy and childbirth. This paper will, therefore, focus on women born between 1950 and 1973[3].

Is this a good IV?

The instrument for schooling, edu_i^* must satisfy the following assumptions in order to produce consistent estimates of the impact of schooling in the model laid out in (15) above:

Assumption 1: $\text{cov}(edu_i^*, \varepsilon_i) = 0$

Assumption 2: $\text{cov}(edu_i^*, edu_i) \neq 0$

In the context of the IV proposed for this paper, the first assumption implies that the interactions between an individual's age in 1974 and program intensity in the region of birth should not be correlated with unobserved determinants in the error term. There is no plausible reason why program intensity in the region of birth would be correlated with unobserved preferences and capabilities that influence women's health care use. The IV uses program intensity in the region of *birth* rather than the region in which primary schooling was completed, in order to minimize bias arising from migration selectivity. Parents migrate strategically to take advantage of a greater availability of schools in

[3] Women born after 1974 are excluded because the programme slowed down considerably after 1978/79. A very high enrolment rate was achieved in 1978. Children born in 1973 would have turned 6 years in 1979 and were, therefore, fully exposed to the programme (Duflo, 2000). Secondly, not all of the women born in 1974 or later would have reached their childbearing years by the 1990s, the period during which my data on pregnancies were collected.

particular areas (Rosenzweig and Wolpin 1988). On the one hand, migration selectivity in turn may be associated with preferences and capabilities described above. The individual's region of birth, on the other hand, is independent of any program related migration, as all of the women in the sample were born before 1974, the year in which the program was launched. Note that this is unlikely to affect the explanatory power of the program effect in any way, as region of birth is highly correlated with the region in which individuals lived when they were 12 years old. Analysis of IFLS migration modules from the 1993 and 2000 waves indicated that over 86% of all women were still living in their region of birth when they were 12 years old.

A key identification assumption is that there are no omitted time varying and region-specific factors correlated with the program. SDIP intensity was higher in less developed regions as the program targeted regions with lower enrollment rates. Mean reversion that would have taken place in poorer regions, even in the absence of SDIP could potentially confound the identification of the education impact. Furthermore, regions which started off at a lower level of development may have modernized faster than others during the 1970s, as a consequence of other targeted development programs initiated by the government following the oil boom. Demand for modern medical care may have increased in less developed regions as a consequence of these other programs, independently of SDIP. It cannot be assumed that changes in health care use patterns across cohorts would have been the same between high and low program regions in the absence of SDIP. I control for this potential source of bias by adding variables to capture time-varying factors correlated with pre-program enrollment rates: enrollment rates in

1972 interacted with age-cohort dummies. Following Duflo (2000;2001), I also add interactions between age-cohort dummies and the allocation of water and sanitation programm, the second largest INPRES program administered at the time.

The fact that younger women were more exposed to modernization and associated preference shifters compared to the oldest women in the sample is another potential source of bias. Moreover, younger women were likely to have pregnancies later on and thus benefit from a range of maternal health-related policy interventions during the 1990s such as the midwife program, which were aimed at encouraging of use of formal health services. Younger women's preference for using formal health services may, therefore, be a consequence of not only their higher level of schooling but also their exposure to modern ideas and improved access to health services. I control for this by including cohort of birth fixed effects and pregnancy-year fixed effects.

The second assumption implies that the instrument is highly correlated with years of schooling and would, therefore, be a good predictor of schooling in the maternal health care model specified in (15) above. I test this assumption in the first stage of my empirical analysis, where I estimate the effect of the school construction program on education.

Selection issues

As explained in the previous section, this paper is concerned with the effects of education on a cohort of women born between 1950 and 1973. Although the survey contains complete fertility histories for the entire cohort of women, data on maternal health care utilization are only available for a sub-sample who had children in the late 1980s and 1990s. Among the cohort of interest, women who had children later are likely to be systematically different to women who had children earlier and whose maternal care choices are not observed in the data used for this analysis. Women's desire to start childbearing earlier may be associated with lower levels of education, as well as other observed and unobserved factors which determine women's fertility and health care choices. Focusing on maternal care choices made by women who gave birth relatively later in life may lead to overestimation of the impact of schooling. I do not explicitly correct for selection bias in the maternal health care analysis but have chosen instead to assess the impact of schooling on the timing of women's fertility decisions. I first estimate the impact of schooling on two related fertility outcomes: age at birth of first child and the probability of having had children by the age of 21. I then estimate the impact of schooling on women's use of maternal care services, conditional on their having had children. The first analysis provides a measure of the extent to which schooling delayed childbearing among the cohort of interest and, consequently, the extent to which the effects of schooling on maternal health care use may be overestimated.

7. Empirical estimation and results

There are three components to my empirical analysis. I begin by estimating the effect of the school construction program on education in order to construct the IV and assess the strength of its relationship with years of schooling. I then estimate the impact of schooling on the timing of women's fertility choices. Finally, I estimate the maternal health care use models specified in Section 6. Both standard probit and IV probit specifications are estimated for the fertility and health care choice models.

Effects of the school construction program on education

The identification strategy outlined above can be generalized to an interaction term analysis. Based on in Duflo (2000; 2001), I begin with the following specification:

$$S_{ijk} = c_1 + \alpha_{1j} + \beta_{1k} + \sum_{l=1}^{23}(P_j \times d_{il})\gamma_{1l} + \sum_{l=2}^{23}(C_j \times d_{il})\delta_{1l} + \eta_{ijk} \qquad (16)$$

where,

S_{ijk} = years of schooling for individual *i*, born in region *j*, in birth cohort *k*

α_{1j} and β_{1k} are region of birth and cohort of birth fixed effects respectively

d_{il} is a dummy that indicates whether individual *i* was age *l* in 1974 (a year of birth dummy)

P_j = program intensity (number of schools built per 1000 children) in region *j*

C_j is a vector of region specific control variables

Each coefficient γ_{1l} may be interpreted as the estimate of the impact of the program for a given cohort. The cohort aged 24 in 1974 forms the control group and is omitted from the regression.

Children aged 13 and older in 1974 were never exposed to the program. Therefore, a testable restriction is that γ_{1l} is small and insignificant for *l*>12 and starts increasing for *l* smaller than some threshold (the oldest age in 1974 when an individual could still benefit from the program). Regression results presented in Table 3 show that estimates for γ_{1l} increase as age in 1974 declines and are more likely to be negative for cohorts aged 13 or older when the program started. Coefficients for the cohort fixed effect are positive and significant for women aged 1 to 12 in 1974; it is small for older women and no longer significant once region-specific controls are included in the model. When coefficient estimates for γ_{1l} are combined with the corresponding cohort fixed effect coefficient, β_{1k}, the overall impact is positive for younger women, particularly those aged 1 to 6 in 1974.

A more efficient specification involves imposing the restriction that the program had no effect on the cohort of women that was never exposed to the program, i.e. l>12. This results in the following specification which is also estimated:

$$S_{ijk} = c_1 + \alpha_{1j} + \beta_{1k} + \sum_{l=1}^{12}(P_j \times d_{il})\gamma_{1l} + \sum_{l=2}^{12}(C_j \times d_{il})\delta_{1l} + \eta_{ijk} \quad (17)$$

The omitted (control) group now comprises women aged 13 to 24 in 1974. This specification leads to more precise estimates of the program effect (Duflo, 2001). Table 4 presents the regression results for this specification. Coefficients for the interaction terms, γ_{1l} combined with the corresponding cohort fixed effect coefficients, β_{1k} are positive and trend upwards with date of birth.

A key requirement needed to ensure that the identification strategy produces unbiased and consistent estimates is that the proposed instrument is correlated with schooling itself. In both specifications all sets of interactions, the key variable measuring program effect, are statistically different from 0. The F-statistics for the null hypothesis of no interaction effects are presented at the bottom of Tables 3 and 4.

The instrument specification to be used in the health care use models presented in the next section includes cohort and region fixed effects, interaction terms and region-specific controls that reflect the pre-program enrollment rate and share of children in the population. Allocation of water and sanitation programs to the region is excluded as it was not significant (jointly and individually) in (16) and (17) above, and had little effect on the program effect terms.

It was noted earlier that this instrument was originally constructed by Duflo (2001) to estimate returns to schooling in Indonesia. Duflo used national intercensal survey data with a sample of 152,989 individuals while I use IFLS data with a sample of 3,921 women. Having a much smaller sample made it necessary to simplify the instrument specification used in my analysis. Duflo's specification controls for *district* of birth and *year* of birth for each woman. Fixed effects in my model control for *region* of birth[4] because there was an insufficient number of individuals within districts to include district fixed effects; *cohort* of birth[5] fixed effects are used instead of year of birth fixed effects for the same reason. Secondly, region-specific controls in Duflo's model consist of characteristics (e.g. enrollment rates) of the district of birth interacted with year of birth; I interact district of birth variables with cohort of birth instead. The interaction term

[4] I grouped the 222 districts in which IFLS respondents were born into 5 regions: Sumatera, Java, Jakarta city, Bali, Nusa, Kalimantan and Sulawesi. These classifications are based on geographical location as well as economic development levels.

[5] I grouped all women into the following cohorts based on age in 1974: 1-12 years, 13-18 years, 19-23 years, and 24 years. The last group was created because it formed the control group in many of the models.

measuring the program effect is the same in both: they interact program intensity in the district of birth with year of birth.

Table 3: Regression results for unrestricted education equation (control group = age 24 years in 1974)

	(1)	(2)	(3)	(4)
Region fixed effects				
Sumatera	1.583	1.5888	1.6151	1.4379
	(0.2472)**	(0.2703)**	(0.2982)**	(0.2954)**
Java	0.3463	0.3381	1.046	0.887
	(0.2302)	(0.2329)	(0.2488)**	(0.2491)**
Jakarta	2.3503	2.289	2.1338	0.0000
	(0.2928)**	(0.2934)**	(0.3043)**	(0.0000)
Bali	0.2558	-0.0337	1.5491	1.5587
	(0.3567)	(0.3599)	(0.3789)**	(0.3768)**
Nusa	-1.0689	-0.9848	0.5742	0.4938
	(0.3134)**	(0.3184)**	(0.3347)	(0.3336)
Kalimantan	0.0633	0.1268	0.7769	0.7123
	(0.3547)	(0.3639)	(0.3812)*	(0.3765)
Cohort of birth fixed effects[(a)]				
Cohort 1: 1-6yrs in 1974	3.6932	4.9285	3.1104	2.9743
	(0.3467)**	(1.5993)**	(1.5580)*	(1.5969)
Cohort 2: 7-12yrs in 1974	2.1566	4.9693	3.0418	2.958
	(0.3761)**	(1.7852)**	(1.7226)	(1.7838)
Cohort 3: 13-18yrs in 1974	1.6457	2.0099	-0.0552	0.614
	(0.3663)**	(1.7359)	(1.6598)	(1.6923)
Cohort 4: 19-23 yrs in 1974	1.4196	-5.7985	-6.9493	-7.0145
	(0.4168)**	(2.0532)**	(1.9685)**	(2.0309)**
Programme intensity x Age in 1974 (interaction terms)				
Age=1	-0.1201	-0.2707	-0.4113	-0.3547
	(0.1358)	(0.1389)	(0.1298)**	(0.1526)*
Age=2	-0.2046	-0.3464	-0.5403	-0.4778
	(0.1246)	(0.1253)**	(0.1540)**	(0.1640)**
Age=3	0.064	-0.0677	-0.2235	-0.177
	(0.1344)	(0.1388)	(0.1335)	(0.1546)
Age=4	-0.0406	-0.2017	-0.379	-0.3229
	(0.1144)	(0.1277)	(0.1249)**	(0.1524)*
Age=5	0.1077	-0.0022	-0.2031	-0.1507
	(0.1250)	(0.1277)	(0.1271)	(0.1495)
Age=6	-0.5426	-0.6883	-0.7858	-0.7202
	(0.1486)**	(0.1518)**	(0.1475)**	(0.1687)**
Age=7	0.1949	0.1437	0.0092	0.0688
	(0.1433)	(0.1442)	(0.1504)	(0.1692)
Age=8	0.3843	0.3372	0.2406	0.3007
	(0.1479)**	(0.1521)*	(0.1506)	(0.1715)
Age=9	-0.2524	-0.3957	-0.4474	-0.3792

	(0.1507)	(0.1449)**	(0.1439)**	(0.1617)*
Age=10	0.1437	0.0577	-0.1581	-0.1093
	(0.1566)	(0.1601)	(0.1898)	(0.1989)
Age=11	-0.6396	-0.6792	-0.7562	-0.6909
	(0.1568)**	(0.1652)**	(0.1721)**	(0.1851)**
Age=12	-0.3649	-0.4058	-0.4807	-0.4042
	(0.1674)*	(0.1702)*	(0.1699)**	(0.1870)*
Age=13	-0.1509	-0.2855	-0.4969	-0.3867
	(0.1727)	(0.1750)	(0.1555)**	(0.1877)*
Age=14	-0.3332	-0.4209	-0.5385	-0.4397
	(0.1524)*	(0.1486)**	(0.1584)**	(0.1758)*
Age=15	-0.193	-0.2429	-0.3929	-0.319
	(0.1346)	(0.1387)	(0.1590)*	(0.1737)
Age=16	-0.6085	-0.6758	-0.7647	-0.6702
	(0.1496)**	(0.1547)**	(0.1547)**	(0.1820)**
Age=17	-0.3009	-0.3477	-0.5509	-0.4466
	(0.1392)*	(0.1401)*	(0.1469)**	(0.1721)**
Age=18	-0.2712	-0.2944	-0.5258	-0.421
	(0.1337)*	(0.1346)*	(0.1495)**	(0.1643)*
Age=19	-0.2699	-0.2241	-0.494	-0.1883
	(0.1805)	(0.1889)	(0.2232)*	(0.2305)
Age=20	-0.0812	-0.0648	-0.3339	-0.0723
	(0.1631)	(0.1603)	(0.1917)	(0.2075)
Age=21	-0.8055	-0.7426	-0.8541	-0.6184
	(0.1673)**	(0.1650)**	(0.1699)**	(0.1904)**
Age=22	-0.282	-0.2323	-0.4691	-0.2098
	(0.2026)	(0.2033)	(0.1900)*	(0.2169)
Age=23	-0.416	-0.3827	-0.5638	-0.2998
	(0.1880)*	(0.1848)*	(0.1969)**	(0.2038)
Pre-programme share of children in the population in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974		-3.7915	-8.2188	-6.762
		(5.7143)	(5.5869)	(5.7790)
Cohort 2: 7-12yrs in 1974		-10.0575	-15.6775	-14.5715
		(6.3190)	(6.1836)*	(6.4845)*
Cohort 3: 13-18yrs in 1974		-0.9922	-2.1929	-3.1051
		(6.1626)	(5.9371)	(6.0909)
Cohort 4: 19-23 yrs in 1974		26.2996	21.3325	23.291
		(7.2262)**	(7.4056)**	(7.5705)**
Pre-programme enrolment rate in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974			20.2123	20.5002
			(1.8857)**	(1.8678)**
Cohort 2: 7-12yrs in 1974			21.9707	22.0907
			(2.4392)**	(2.3538)**
Cohort 3: 13-18yrs in 1974			15.6412	17.6751
			(2.0639)**	(2.0431)**
Cohort 4: 19-23 yrs in 1974			17.0686	18.7945

			(2.9283)**	(2.7950)**
Allocation of water and sanitation programmes in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974				-0.5387
				(0.7051)
Cohort 2: 7-12yrs in 1974				-0.4789
				(0.8665)
Cohort 3: 13-18yrs in 1974				-1.5619
				(0.8907)
Cohort 4: 19-23 yrs in 1974				-2.3435
				(1.0646)*
Constant	3.7477	3.7898	3.2793	3.2523
	(0.3506)**	(0.3554)**	(0.3736)**	(0.3734)**
Observations	5,875	5,820	5,788	5,439
R-squared	0.14	0.14	0.2	0.19
Ftest: all interactions=0[(b)]	5.81	5.79	5.75	4.73
Prob>F	0.0000	0.0000	0.0000	0.0000

Notes:
(a) The control group was women aged 24 in 1974
(b) F-test to test the hypothesis that interaction coefficients between cohort of birth and programme intensity in the region of birth are jointly zero
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Table 4: Regression results for restricted education equation (control group = age 13-24 years in 1974)

	(1)	(2)	(3)	(4)
Region fixed effects				
Sumatera	1.5005	1.6727	1.7209	1.7287
	(0.2477)**	(0.2577)**	(0.2730)**	(0.2724)**
Java	0.3285	0.3738	0.7512	0.7531
	(0.2316)	(0.2328)	(0.2420)**	(0.2426)**
Jakarta	2.4647	2.4187	2.2673	0.0000
	(0.2914)**	(0.2917)**	(0.2992)**	(0.0000)
Bali	-0.1511	-0.5104	0.1959	0.1968
	(0.3440)	(0.3468)	(0.3592)	(0.3594)
Nusa	-1.2114	-1.0971	-0.2188	-0.2163
	(0.3131)**	(0.3183)**	(0.3295)	(0.3295)
Kalimantan	-0.1668	0.0007	0.3014	0.3171
	(0.3525)	(0.3566)	(0.3656)	(0.3646)
Cohort of birth fixed effects[(a)]				
Cohort 1: 1-6yrs in 1974	2.7996	4.6724	4.1468	4.4485
	(0.2154)**	(1.5415)**	(1.4808)**	(1.5222)**
Cohort 2: 7-12 yrs in 1974	1.2512	4.6576	4.0299	4.3832
	(0.2576)**	(1.7317)**	(1.6561)*	(1.7207)*
Programme intensity x Age in 1974 (interaction terms)				
Age=1	-0.0648	-0.2221	-0.2941	-0.3033
	(0.1351)	(0.1376)	(0.1291)*	(0.1512)*
Age=2	-0.1534	-0.3064	-0.4501	-0.4605
	(0.1246)	(0.1239)*	(0.1503)**	(0.1608)**
Age=3	0.1208	-0.0162	-0.1061	-0.1229
	(0.1324)	(0.1360)	(0.1294)	(0.1504)
Age=4	0.0097	-0.1606	-0.2812	-0.2951
	(0.1123)	(0.1239)	(0.1174)*	(0.1460)*
Age=5	0.1549	0.0365	-0.1097	-0.1227
	(0.1243)	(0.1262)	(0.1248)	(0.1471)
Age=6	-0.4876	-0.6406	-0.674	-0.6747
	(0.1480)**	(0.1508)**	(0.1456)**	(0.1678)**
Age=7	0.2518	0.1939	0.1191	0.1085
	(0.1421)	(0.1427)	(0.1486)	(0.1685)
Age=8	0.4379	0.3832	0.3359	0.3259
	(0.1482)**	(0.1525)*	(0.1538)*	(0.1752)
Age=9	-0.1957	-0.3481	-0.3511	-0.3518
	(0.1499)	(0.1428)*	(0.1417)*	(0.1595)*
Age=10	0.196	0.1008	-0.064	-0.0898
	(0.1567)	(0.1598)	(0.1882)	(0.1996)
Age=11	-0.5769	-0.622	-0.6398	-0.6443

	(0.1524)**	(0.1600)**	(0.1625)**	(0.1773)**
Age=12	-0.3023	-0.35	-0.3653	-0.3586
	(0.1657)	(0.1683)*	(0.1672)*	(0.1854)
Pre-programme share of children in the population in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974		-5.9802	-14.3012	-15.1135
		(5.6076)	(5.4523)**	(5.6417)**
Cohort 2: 7-12 yrs in 1974		-12.0803	-21.5738	-22.8432
		(6.2204)	(6.0806)**	(6.3838)**
Pre-programme enrollment rate in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974			18.287	18.3632
			(1.8483)**	(1.8411)**
Cohort 2: 7-12 yrs in 1974			20.0913	20.0457
			(2.4157)**	(2.3670)**
Allocation of water and sanitation programmes in region of birth x birth cohort[(a)]				
Cohort 1: 1-6yrs in 1974				-0.0863
				(0.6884)
Cohort 2: 7-12 yrs in 1974				0.0444
				(0.8727)
Constant	4.5886	4.5317	4.2357	4.2181
	(0.2287)**	(0.2307)**	(0.2406)**	(0.2414)**
Observations	5,875	5,820	5,788	5,439
R-squared	0.13	0.13	0.17	0.15
Ftest: interactions=0[(b)]	7.43	7.40	6.71	6.32
Prob>F	0.0000	0.0000	0.0000	0.0000

Notes
(a) The control group was women aged 13-24 in 1974
(b) F-test to test the hypothesis that interaction coefficients between cohort of birth and programme intensity in the region of birth are jointly zero
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Effects of female schooling on fertility

Table 5 presents regression estimates of the impact of schooling on two types of fertility outcomes: the probability of having had children by the age of 21 and age at first birth. Schooling has a negative impact on the probability of having children before the age of 21. The magnitude of the coefficient for schooling is similar in both the IV-probit and standard-probit specifications, although statistically significant in only the latter. Regressions on age at first birth confirm that increased schooling leads to later childbearing. The coefficients are similar in magnitude in both the IV and OLS specifications, but statistically significant only in the OLS model. The lack of statistical significance in the IV models is not surprising as IV estimates lack precision relative to OLS or standard probit estimates.

Coefficients for age at first marriage are strongly significant and in the anticipated direction in both models. As mentioned earlier, only a small proportion of births take place out of wedlock in Indonesia. An increase in age at first marriage is, therefore, an important factor in delaying childbearing among women. Consistent with findings in the fertility literature, higher household socioeconomic status as measured by consumption per capita is associated with later age at first birth. Statistically significant coefficients for all of the region dummies highlight sharp geographic variations within Indonesia in age at first birth. Overall, the models perform quite well as indicated by the Wald chi-squared statistic for the probit models and R-squared for the OLS models.

Table 5: Impact of schooling on fertility choices

	Probability of having had a child by age 21		**Age at first birth**	
	IV-PROBIT	**PROBIT**	**IV**	**OLS**
Years of schooling	-0.0777	-0.0583	0.0794	0.0735
	(0.0650)	(0.0073)**	(0.0863)	(0.0149)**
Log of household consumption per capita (Rp.'000000)	-0.0001	-0.0003	0.0008	0.0008
	(0.0007)	(0.0003)	(0.0009)	(0.0004)*
Age at first marriage	-0.2237	-0.2295	0.7149	0.7164
	(0.0243)**	(0.0140)**	(0.0291)**	(0.0184)**
Urban area of residence	0.0945	0.0669	-0.1457	-0.1370
	(0.1080)	(0.0496)	(0.1505)	(0.0849)
Travel time to nearest hc/mw - hrs	-0.1271	-0.1108	0.1208	0.1157
	(0.0802)	(0.0598)	(0.1199)	(0.0956)
Region fixed effects				
Sumatera	0.2370	0.2231	-0.6556	-0.6512
	(0.1216)	(0.1126)*	(0.1916)**	(0.1828)**
Java	0.0990	0.0968	-0.4470	-0.4464
	(0.1053)	(0.1049)	(0.1731)**	(0.1725)**
Jakarta	0.3303	0.3243	-0.6455	-0.6432
	(0.1286)**	(0.1272)*	(0.2011)**	(0.1985)**
Bali	-0.0787	-0.0594	-0.3609	-0.3665
	(0.1594)	(0.1473)	(0.2513)	(0.2368)
Nusa	0.1399	0.1569	-0.7377	-0.7427
	(0.1538)	(0.1425)	(0.2478)**	(0.2347)**
Kalimantan	0.2460	0.2533	-0.9400	-0.9420
	(0.1457)	(0.1434)	(0.2451)**	(0.2434)**
Cohort of birth fixed effects				
Cohort 1: 1-6yrs in 1974	1.3793	1.3511	-1.8993	-1.8899
	(0.6099)*	(0.6021)*	(0.8558)*	(0.8429)*
Cohort 2: 7-12 yrs in 1974	0.0097	0.0084	-1.2085	-1.2060
	(0.6407)	(0.6415)	(1.0362)	(1.0341)
Pre-programme share of children in the population in region of birth x birth cohort				
Cohort 1: 1-6yrs in 1974	-5.0066	-4.9542	6.0467	6.0288
	(2.2500)*	(2.2417)*	(3.0928)*	(3.0814)
Cohort 2: 7-12 yrs in 1974	0.0680	0.1374	3.2455	3.2184
	(2.4447)	(2.4281)	(3.9431)	(3.9188)
Pre-programme enrolment rate in region of birth x birth cohort				
Cohort 1: 1-6yrs in 1974	0.2301	0.0735	0.4098	0.4569
	(0.7765)	(0.5958)	(1.0825)	(0.8975)
Cohort 2: 7-12 yrs in 1974	-0.1260	-0.3254	1.3141	1.3727
	(1.0442)	(0.8573)	(1.5800)	(1.4404)

Constant	4.5534	4.5816	7.4744	7.4705
	(0.2673)**	(0.2463)**	(0.3252)**	(0.3186)**
Observations	5356	5356	5356	5356
R-squared			0.6521	0.6521
Model Wald chi-squared	680.29	909.58		
Prob > chi-squared	0.0000	0.0000		

Notes
Robust standard errors in parentheses

Effects of female schooling maternal health care use

Timing of ante-natal care visits

Table 6 contains regression estimates of the probability of a woman receiving ante-natal care during the first trimester of her pregnancy. Schooling has a positive, statistically significant impact on the use of ante-natal care early in the pregnancy. Each additional year of schooling for a woman with average education (6.7 years) increases the probability of her having at least one ante-natal care visit during the first trimester by 6.6% in the IV-probit model. The corresponding marginal probability estimate in the standard probit model is 2.3%. These findings are not consistent with the hypothesis that standard regression estimates of schooling are overestimated due to omitted variable bias. Rather, they lend support to the hypothesis that measurement error leads to underestimation of the impact of schooling.

Household income as proxied by annual consumption per capita and urban area of residence also have a strong positive impact on the probability of receiving ante-natal care early in the pregnancy because they imply fewer financial and physical barriers to access. Travel time to the nearest health center or midwife, another measure of access to care has a significant negative impact. For instance, an increase of 1 hour in the average travel time to the nearest health care provider (0.5 hours) reduces the probability of receiving ante-natal care by 1.9%. The direction of the coefficients for each of these three covariates is consistent in both specifications, although statistically significant only in the standard-probit specification. Time trend coefficients are positive and statistically significant, indicating that pregnancies occurring in the late 1990s were much more likely to benefit from early ante-natal care than those prior to 1990. A government program to expand midwifery services began in 1994 and targeted ante-natal care provision amongst other pregnancy related services. It has been shown elsewhere that this program did indeed lead to an increase in the use of formal health services for pregnancy care in Indonesia (Frankenberg 2005).

With the exception of the coefficient for schooling, region fixed effects and time trends, none of the other variables are statistically significant in the IV-probit model. Large standard errors are consistent with the lower level of precision associated with IV estimates. It is important, therefore, to confirm that endogeneity bias is present in this model, which requires use of an IV. A Haussman test carried out on the standard probit and IV-probit models rejects the null hypothesis that no endogeneity is present in the standard probit model (p=0.0004), as the chi-squared statistic reported in Table 6 indicates. This justifies use of the IV for the analysis of ante-natal care.

Table 6: Regression estimates of the probability of seeking ante-natal care

	IV-PROBIT	PROBIT
Years of schooling	0.2025	0.0754
	(0.0822)**	(0.0078)**
Log of household consumption per capita (Rp.'000000)	0.0007	0.0026
	(0.0016)	(0.0007)**
Age at childbirth	-0.0028	0.0038
	(0.0109)	(0.0104)
Parity (reference group: first birth)		
Low parity: 2nd or 3rd birth	0.1206	0.0649
	(0.1042)	(0.1020)
Medium parity: 4th or 5th birth	0.1609	-0.1709
	(0.2665)	(0.1135)
High parity: 6th birth or higher	-0.1029	-0.5539
	(0.3732)	(0.1305)**
Outcome of previous pregnancy: alive	0.1186	-0.074
	(0.1619)	(0.0903)
Urban area of residence	0.0348	0.2738
	(0.1913)	(0.0542)**
Time to nearest health centre or midwife - hrs	-0.0575	-0.1792
	(0.1064)	(0.0622)**
Year of childbirth (reference group: 1985-89)		
1990-1994	0.1364	0.2162
	(0.0942)	(0.0637)**
1995-1997	0.4164	0.6118
	(0.2034)*	(0.1050)**
1998-2000	0.4517	0.6333
	(0.2161)*	(0.1419)**
Region fixed effects		
Sumatera	0.4830	0.6259
	(0.1840)**	(0.1229)**
Java	0.6783	0.7342
	(0.14104)**	(0.1119)**
Jakarta	0.6122	0.7022
	(0.1880)**	(0.1607)**
Bali	1.3109	1.3481
	(0.2170)**	(0.1849)**
Nusa	0.4532	0.4374
	(0.1369)**	(0.1397)**
Kalimantan	0.3172	0.2871
	(0.1557)*	(0.1602)
Cohort of birth fixed effects		
Cohort 1: 1-6yrs in 1974	0.4651	0.321
	(0.7507)	(0.7825)

Cohort 2: 7-12 yrs in 1974	1.0610	0.9638
	(0.7374)	(0.7635)
Pre-programme share of children in the population in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	-1.7678	-1.8968
	(2.7502)	(2.9072)
Cohort 2: 7-12yrs in 1974	-4.1393	-4.7471
	(2.8350)	(2.8524)
Pre-programme enrolment rate in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	-0.2721	0.9329
	(1.1090)	(0.7740)
Cohort 2: 7-12 yrs in 1974	0.2104	1.8596
	(1.5363)	(0.8217)*
Constant	-1.5580	-0.9567
	(0.4792)**	(0.3435)**
Observations	3648	3648
Model Wald chi-squared[(a)]	874.65	618.5
Prob > chi-squared	0.0000	0.0000
Haussman specification test for endogeneity[(b)]		
chi-squared	54.61	
Prob > chi-squared	0.0004	

Notes:
(a) Wald statistics and p value for the test of the hypothesis that all of the slope coefficients are jointly zer0
(b) Haussman test of the null hypothesis that both the standard probit and IV probit estimators are consistent.
Robust standard errors in parentheses; * significant at 5%; ** significant at 1%

Skilled assistance at delivery

Regression estimates of the probability of having skilled assistance at delivery are presented in Table 7. The coefficient for schooling is positive and statistically significant. The marginal effect of an additional year of schooling on the probability of having skilled assistance at delivery is 12.2% for a woman with average education. As in the ante-natal care model, the marginal effect is smaller in magnitude in the standard probit model.

Urban area of residence, travel to nearest midwife or health center and annual household consumption per capita have a priori expected signs: positive in the case of consumption and urban area of residence and negative for travel time. They are not significant in the IV probit models, however. The coefficient for age at childbirth is positive, indicating that older women are more likely to seek skilled assistance. Women having high order pregnancies are also more likely to seek out skilled assistance for delivery. Time trends associated with the year in which the pregnancy took place are positive. This trend is attributable in part to the expansion of midwifery services in the late 1990s.

Haussman test results reported in Table 7 once again reject the null hypothesis of no endogeneity in the standard probit model (p=0.0000). The use of an IV is, therefore, justified for the skilled assistance model.

Table 7: Regression estimates of the probability of having skilled assistance at delivery

	IV-PROBIT	**PROBIT**
Years of schooling	0.3067	0.1324
	(0.0319)**	(0.0081)**
Log of household consumption per capita (Rp.'000000)	-0.0008	0.0022
	(0.0010)	(0.0006)**
Age at childbirth	0.0241	0.0447
	(0.0131)	(0.0111)**
Parity (reference group: first birth)		
Low parity: 2nd or 3rd birth	0.1295	0.0433
	(0.0879)	(0.0983)
Medium parity: 4th or 5th birth	0.4473	-0.0493
	(0.1619)**	(0.1139)
High parity: 6th birth or higher	0.4678	-0.1844
	(0.2134)*	(0.1400)
Outcome of previous pregnancy: alive	0.1529	-0.1768
	(0.1192)	(0.0908)
Urban area of residence	0.2075	0.7043
	(0.1755)	(0.0539)**
Time to nearest health centre or midwife - hrs	0.0016	-0.2097
	(0.0806)	(0.0881)*
Year of childbirth (reference group: 1985-89)		
1990-1994	0.0284	0.1576
	(0.0743)	(0.0695)*
1995-1997	0.2986	0.6662
	(0.1675)	(0.1091)**
1998-2000	0.3203	0.6627
	(0.1793)	(0.1436)**
Region fixed effects		
Sumatera	0.2206	0.452
	(0.1544)	(0.1362)**
Java	0.2505	0.2976
	(0.1189)*	(0.1248)*
Jakarta	0.6353	0.8821
	(0.2024)**	(0.1809)**
Bali	1.3032	1.5349
	(0.2300)**	(0.1804)**
Nusa	-0.1494	-0.3183
	(0.1622)	(0.1664)
Kalimantan	-0.0129	-0.1384
	(0.1661)	(0.1837)
Cohort of birth fixed effects		
Cohort 1: 1-6yrs in 1974	0.8915	0.7712
	(0.6941)	(0.7938)

Cohort 2: 7-12 yrs in 1974	1.5618	1.6805
	(0.7125)*	(0.7840)*
Pre-programme share of children in the population in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	-3.5010	-4.3169
	(2.6206)	(2.8959)
Cohort 2: 7-12 yrs in 1974	-5.5211	-7.5318
	(2.7520)*	(2.9118)**
Pre-programme enrolment rate in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	0.3892	2.8362
	(0.9844)	(0.7983)**
Cohort 2: 7-12 yrs in 1974	-0.2422	2.6035
	(1.1613)	(0.9028)**
Constant	-3.4222	-3.118
	(0.3712)**	(0.3714)**
Observations	3648	3648
Model Wald chi-squared[(a)]	2747.69	1111.92
Prob > chi-squared	0.0000	0.0000
Hausman specification test for endogeneity[(b)]		
chi-squared	261.77	
Prob > chi-squared	0.0000	

Notes:
(a) Wald statistics and p value for the test of the hypothesis that all of the slope coefficients are jointly zero
(b) Haussman test of the null hypothesis that both the standard probit and IV probit estimators are consistent.
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Hospital deliveries

Regression estimates for the probability of a woman delivering her child in a hospital are presented in Table 8. The coefficient for schooling is positive, but significant only in the standard probit model. An additional year of schooling increases the probability of a woman with average education going to a hospital for child delivery by 4% in the IV-probit model. The marginal effect is only 1.85% in the standard probit model.

Hospital care is relatively expensive in Indonesia because of the absence of adequate insurance or other forms of health care safety nets. Household consumption per capita, a measure of the extent to which women can afford hospital care is a positive and significant predictor of having a hospital delivery. With hospitals located almost exclusively in urban areas, urban area of residence is associated with a high probability of delivering in a hospital. Neither coefficient is statistically significant in the IV-probit model. The coefficients for time trends which were important in the ante-natal care and skilled assistance models, are not significantly different from zero in the hospital care model.

The Haussman test reported in Table 8 fails to reject the null of no endogeneity in the standard probit model (p=1.0000). An IV for schooling may, therefore, not be appropriate for the analysis of hospital delivery care.

Table 8: Regression estimates of the probability of delivering in a hospital

	IV-PROBIT	PROBIT
Years of schooling	0.1908	0.095
	(0.1056)	(0.0092)**
Log of household consumption per capita (Rp.'000000)	0.0002	0.0015
	(0.0016)	(0.0004)**
Age at childbirth	0.0206	0.0267
	(0.0148)	(0.0122)*
Parity (reference group: first birth)		
Low parity: 2nd or 3rd birth	0.0173	-0.0368
	(0.1182)	(0.1030)
Medium parity: 4th or 5th birth	0.1322	-0.1122
	(0.3165)	(0.1259)
High parity: 6th birth or higher	0.1270	-0.1805
	(0.4014)	(0.1576)
Outcome of previous pregnancy: alive	-0.0583	-0.2065
	(0.2093)	(0.0966)*
Urban area of residence	0.2808	0.464
	(0.2489)	(0.0618)**
Time to nearest health centre or midwife - hrs	0.0325	-0.0486
	(0.1240)	(0.0887)
Year of childbirth (reference group: 1985-89)		
1990-1994	-0.1003	-0.0574
	(0.0897)	(0.0800)
1995-1997	-0.1035	-0.0009
	(0.1621)	(0.1152)
1998-2000	-0.1150	-0.0282
	(0.1767)	(0.1501)
Region fixed effects		
Sumatera	-0.2713	-0.2195
	(0.1402)	(0.1382)
Java	-0.2044	-0.2265
	(0.1243)	(0.1250)
Jakarta	0.0256	0.046
	(0.1517)	(0.1545)
Bali	0.3653	0.3151
	(0.1838)*	(0.1849)
Nusa	-0.2762	-0.3371
	(0.1918)	(0.1799)
Kalimantan	-0.1821	-0.2323
	(0.1926)	(0.1885)
Cohort of birth fixed effects		
Cohort 1: 1-6yrs in 1974	-0.3617	-0.5042
	(0.7611)	(0.7524)

Cohort 2: 7-12 yrs in 1974	1.0381	0.9746
	(0.7728)	(0.7757)
Pre-programme share of children in the population in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	0.2220	0.2175
	(2.6827)	(2.7392)
Cohort 2: 7-12 yrs in 1974	-4.7321	-5.1736
	(3.0088)	(2.8828)
Pre-programme enrolment rate in region of birth x birth cohort		
Cohort 1: 1-6yrs in 1974	0.5200	1.4728
	(1.3412)	(0.6916)*
Cohort 2: 7-12 yrs in 1974	1.0195	2.2119
	(1.7073)	(0.8579)**
Constant	-2.7583	-2.3658
	(0.4857)**	(0.3961)**
Observations	3648	3648
Model Wald chi-squared[(a)]	629.55	517.23
Prob > chi-squared	0.0000	0.0000
Wald test of exogeneity[(b)]		
chi-squared	0.65	
Prob > chi-squared	0.4214	
Hausman specification test for endogeneity[(b)]		
chi-squared	4.5	
Prob > chi-squared	1.0000	

Notes:
(a) Wald statistics and p value for the test of the hypothesis that all of the slope coefficients are jointly zero
(b) Haussman test of the null hypothesis that both the standard probit and IV-probit estimators are consistent.
Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

8. Discussion

The schooling effect

To summarize, the IV method identifies the effect of giving one more year of education to a randomly selected woman before she started having children. Table 9 provides a summary of the marginal effects of an additional year of schooling on the fertility and maternal care outcomes examined in this paper. It is clear that schooling increases the age at which women begin childbearing. The estimated effects of schooling on fertility choices are not significant in the IV models, however. Women's schooling is significantly non-zero in each of the three maternal health care relationships, with a positive sign as hypothesized in each case. In all three cases, the IV estimate of the schooling effect is considerably larger than the standard probit estimate. With the exception of the hospital care model, the null hypothesis of no endogeneity is rejected in all cases, providing justification for use of the IV for schooling.

Table 9: Marginal effects of schooling on fertility and maternal care use outcomes

Dependent variable	**IV-PROBIT**	**PROBIT**
Probability of having a child before age 21	-0.0310	-0.0232
Age at first birth	0.0794	0.0735
Probability of seeking ante-natal care in the first trimester	0.0666	0.0238
Probability of delivering in a hospital	0.0400	0.0185
Probability of having skilled assistance at delivery	0.1222	0.0527

Notes:
Marginal effects calculated at the mean values of the schooling variable after probit or ivprobit estimation. The marginal effect represents the change in the outcome in response to one additional year of schooling, at the mean level of schooling of 6.2 years

The maternal care analysis excludes a sub-sample of women who completed their childbearing fairly early on and were not observed in the survey. The analysis of fertility outcomes indicates that more educated women are indeed more likely to start childbearing later. It is, therefore, reasonable to conclude that the maternal care analysis presented in this paper overestimates the actual impact of schooling by focusing on a sub-sample of women who chose to start childbearing later due to the influence of schooling and other factors associated with modernization and change. Estimation of the impact of schooling on maternal health care use for the entire cohort of women with corrections for selection bias is beyond the scope of this paper. However, the estimated effects of schooling on maternal care use are so large that they are not likely to be negated even if selection issues in the data were to be accounted for.

This analysis confirms existing knowledge about the relationship between education and maternal health care use. It also provides new estimates of the schooling effect that control for heterogeneity in unobserved endowments and preferences among women. The observed effects of education on health care use do not simply reflect health knowledge and the ability to process information and ideas that are learnt at school; they also reflect preferences of the household and community in which the individual grew up, and health related skills and abilities that are acquired during childhood. Previous studies that addressed the issue of omitted variable bias in women's schooling and health equations did so by including controls for unobserved childhood background related ability and motivation (Behrman and Wolfe 1987; Wolfe and Behrman 1987). They conclude that schooling does appear to affect women's health and nutrition even with controls for unobserved childhood background characteristics. Other studies controlled for potential confounders associated with a woman's household and community to find that the schooling effect is reduced but by no means eliminated. I use an IV approach in this paper to obtain more robust estimates of the schooling effect than those obtained by controlling for background characteristics alone. Results presented in this paper reinforce existing evidence that schooling has a quantitatively important effect on the maternal health care use, although the IV parameter estimates lack precision in most cases.

Methodological concerns: a poor IV for schooling?

It is worth noting that in all of the models the IV estimates of schooling are considerably larger than the standard-probit estimates. One possible explanation for this is that the standard regression estimates of schooling are biased towards zero because of measurement error in the years of schooling variable. IV estimates, which are not subject to this type of measurement are, therefore, larger. An alternative explanation for the large IV estimates is that the IV itself is of poor quality. It was explained earlier that the schooling IV, edu^* must satisfy two assumptions in order to produce unbiased and consistent estimates of the schooling effect: it must be uncorrelated with error terms, ε_i in the structural model ($\mathrm{cov}(edu_i^*, \varepsilon_i) = 0$) and it must be highly correlated with schooling, edu itself ($\mathrm{cov}(edu_i^*, edu_i) \neq 0$). In Section 6, I provide reasons why the first assumption is likely to be satisfied and account for any remaining sources of bias in the specification of the IV. The second assumption is tested in Section 7 by estimating schooling equations (16)-(17). Results presented in Tables 3 and 4 show that the proposed set of instruments, although jointly significant do not explain a substantial proportion of the variation in schooling ($R^2 < 0.2$). IV estimates of schooling may be upwardly biased as a result.

Weak correlation between the IV and schooling variable has other negative implications. The IV estimator may have large asymptotic bias even if the IV is only slightly correlated with the error term. This is shown by deriving the probability limit of the IV estimator in terms of population correlations and standard deviations, given possible correlation between the IV and the error term as follows (Wooldridge 2000):

$$p\lim \beta^{IV} = \beta + \frac{Corr(edu^*, \varepsilon)}{Corr(edu^*, edu)} \cdot \frac{\sigma_\varepsilon}{\sigma_{edu}}, \tag{18}$$

where, σ_ε and σ_{edu} are the standard deviations of ε and edu in the population, respectively. It shows that even if $Corr(edu^*, \varepsilon)$ is small, inconsistency in the IV estimator can be very large if $Corr(edu^*, edu)$ is also small. By contrast, the probability limit of the ordinary least squares estimator given correlation between schooling and the error terms, is as follows[6]:

$$p\lim \tilde{\beta}^{OLS} = \beta + Corr(edu, \varepsilon) \cdot \frac{\sigma_\varepsilon}{\sigma_{edu}} \tag{19}$$

Comparison of (18) and (19) shows that where correlation between the IV and schooling is weak and consequently, $\frac{Corr(edu^*, \varepsilon)}{Corr(edu^*, edu)} > Corr(edu, \varepsilon)$, the OLS estimator of schooling (in this case the probit equivalent), may be preferred to the IV estimator on asymptotic bias grounds (Wooldridge 2000).

The effects of other key determinants of maternal health care use

This analysis has also highlighted several other important predictors of use of maternal health services. Older women are more likely to seek ante-natal care in the first trimester

[6] This is derived by using the following:

$Corr(edu, \varepsilon) = \frac{Cov(edu, \varepsilon)}{(\sigma_{edu} \cdot \sigma_\varepsilon)}$ and

$p\lim \hat{\beta} = \beta_1 + \frac{Cov(edu, \varepsilon)}{Var(edu)}$

and have skilled assistance at delivery. Maternal age thus serves as a proxy for the woman's accumulated knowledge of health care services and the value she places on modern medicine (Elo 1992). Having controlled for maternal age, women are more likely to seek maternal health services for first, rather than high order births. This is consistent with findings in Peru by Elo, who argues that multiparous women may attach less importance to pregnancy and childbirth having experienced it more than once. This is also consistent with my other finding that women with previous pregnancies that ended in live outcomes are less likely to seek maternal care. Having several other small children at home and difficulties in finding care-givers for them may also deter multiparous women from seeking care outside. In urban areas of the Philippines, Wong et al (1987) found that an increase in the number of young children in the family had a negative effect on use of ante-natal care services.

Consistent with previous work in this area (Wong, Popkin et al. 1987; Elo 1992; Hodgkin 1996), my findings suggest that access to health care, as measured by urban area of residence and travel time to the nearest provider, are quantitatively important and statistically significant in predicting health care use. For instance, a significant proportion of women in IFLS with 2 years or less of schooling living in urban areas in Java and Bali received ante-natal care in the first trimester (72%) and had skilled assistance at delivery (45%); comparable ratios for women who had 6 or more years of schooling in rural Sulawesi and Nusa were 56% and 19% respectively. Increased education is unlikely to bring about substantial behavioral change in women's use of maternal health services if access to services remains limited in remote, rural areas. Financial barriers to access as measured by household consumption per capita are also important for all three types of maternal health care use examined, although they are not always statistically significant in the IV models. An important conclusion to draw from this analysis is that education, while important for expanding demand for health services, may have only a limited impact if significant other barriers to access exist, which inhibit women from seeking pregnancy and childbirth care in the formal sector.

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